decimals, though little significance can be attached to the last figure. Lines which are apparently diffuse are characterised by the letter *d*.

In conclusion, I should like to express my great indebtedness to Sir William Ramsay for allowing me the use of the emanation, and for purifying it on all occasions. My best thanks are also due to Dr. Gray for assisting in the purification of the emanation and the subsequent manipulation, and to Mr. Cameron for placing at my disposal all his original measurements.

On the Perception of the Direction of Sound.

By Lord Rayleigh, O.M., F.R.S.

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The nature of the clue by which we are able to pronounce whether a sound of low pitch reaches us from the right or from the left was long a mystery, seeing that in such cases the difference of intensities at the two ears, used singly, is inappreciable. By some special laboratory experiments conducted about three years ago,[*] I was able to show that the discrimination depends upon the *phase*-difference at the two ears, and that the sound is judged to be on that side where the phase is in *advance*. When the pitch is higher (much above *g′*) no distinct lateral effect accompanies a phase-difference, and the discrimination of right and left in ordinary hearing undoubtedly depends upon intensities. Commenting on these results, I remarked (p. 224): "The conclusion, no longer to be resisted, that when a sound of low pitch reaches the two ears with approximately equal intensities, but with a phase-difference of a quarter of a period, we are able so easily to distinguish at which ear the phase is in advance, must have far-reaching consequences in the theory of audition. It seems no longer possible to hold that the vibratory character of sound terminates at the outer ends of the nerves along which the communication with the brain is established. On the contrary, the processes in the nerve must themselves be vibratory, not, of course, in the gross mechanical sense, but with preservation of the period and retaining the characteristic of phase—a view advocated by Rutherford, in opposition to Helmholtz, as long ago as 1886."

In the 'Proceedings'[†] for January, 1908, Profs. Myers and Wilson detail some interesting experiments, made by another method, which confirm my general conclusion that phase-differences at the outer ears are recognised and give rise to "lateral sensations," but they put a different interpretation upon the fact from that which I suggested in the paragraph above quoted In their view the ultimate cause of the lateral effect is still a difference of intensities at the *inner* ears which may occur, in spite of equality at the *outer* ears, in virtue of conduction of sound from one side to the other through the bones of the head. I had already (p. 227) considered the somewhat similar question of the consequences of conduction through the air round the head, and had shown that the observed effects could not be explained upon the intensity theory as the result of such conduction. At first sight there would seem to be but little difference between the two suppositions, and the opposite character of the conclusions depends, in fact, upon the introduction by Profs. Myers and Wilson of a phase-reversal. It may be well to quote their own statement* of the assumptions upon which they base their explanation. These are: "(*a*) that the sound entering one ear is transmitted through the bones of the head to the opposite inner ear; (*b*) that the retardation of phase due to such interaural conduction is small; and (*c*) that the two sets of waves, thus received at each ear directly and by bone conduction, meet coming from opposite directions." It is this arrival from opposite directions which is considered to entail a phase-reversal, and the required conclusion then readily follows. It will be convenient to generalise somewhat the algebraical statement.

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[*] 'Phil. Mag.,' vol. 13, p. 214, 1907.

[†] Vol. 80, A, p. 260, 1908; see also 'Journal of Psychology,' vol. 2, October, 1908.

Let $y_1 = a\sin(\omega t+\alpha)$ denote the vibration entering the right ear, and $y_2 = b\sin\omega t$ denote the vibration entering the left ear, so that α is the angle (less than π) by which the vibration at the right outer ear is in advance. The resulting effect at the right internal ear may be taken to be

$$y_1' = fa\sin(\omega t+\alpha)-gb\sin(\omega t-\beta).$$

Here f and g are proper fractions, of which f is much greater than g, and β is the retardation in phase due to the passage through the head. The negative sign of the second term expresses the phase-reversal, so that the whole retardation is really $\pi+\beta$. In the same way the effect at the left internal ear will be

$$y_2' = fb\sin\omega t-ga\sin(\omega t+\alpha-\beta).$$

Let I_1 denote the sound intensity at the right internal ear, and I_2 that at the left internal ear. Then

$$I_1 = f^2a^2+g^2b^2-2fgab\cos(\alpha+\beta),$$

$$I_2 = f^2b^2+g^2a^2-2fgab\cos(\alpha-\beta);$$

and

$$I_1-I_2 = (f^2-g^2)(a^2-b^2)+4fgab\sin\alpha\sin\beta. \qquad (1)$$

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* 'Journn. Psych.,' *loc. cit.*, p. 380.

If $b = a$, this reduces to the value given by Myers and Wilson, viz.,

$$I_1 - I_2 = 4fga^2 \sin \alpha \sin \beta. \tag{2}$$

In these expressions $\sin \alpha$ is supposed to be positive, that is the vibration at the right outer ear *leads;* and we see that $I_1 > I_2$, if β be positive and less than π. This conclusion depends, of course, upon the assumed reversal of phase, and would itself be reversed if β were the whole retardation incurred.

The question thus raised is of considerable interest. It must be admitted at once that simplicity is on the side of a theory that would attribute the lateral sensation in all cases to a difference of intensity at the two inner ears. Myers and Wilson are evidently much influenced, as I also was, by an *a priori* unwillingness to admit the possibility of a phase-difference being appreciated directly. But I was under the impression that this difficulty would be less felt by physiologists now than in the days of Helmholtz.

When we come to scrutinise closely the intensity theory, as put forward by Myers and Wilson, we meet a good many difficulties, though perhaps none of them are insuperable. It would seem that the positive effect of the second term of (1), where g is very small and β is somewhat small, would be easily overpowered by the first term if b slightly exceeded a, especially if $\sin \alpha$ is also somewhat small. In my experiments, where two tones nearly in unison were led separately to the two ears, the lateral sensation changed over somewhat suddenly from left to right, or *vice versâ*, as the phase-difference passed through zero or 180°. And although the best effects required some regulation of the intensities, it did not appear that the adjustment needed to be at all precise. Myers and Wilson narrate a curious observation in which the intensities at the two outer ears were made unequal. The influence of the change seemed not to be permanent. "We see, then, how rapidly we become adapted to binaural differences of intensity, how susceptible we still are to phase-differences after such adaptation has been established, and how such adaptation, once acquired, persists for a short time after the sound intensity at the two ears has been equalised."

I had already (*loc. cit.*, p. 217) tried an experiment in order to see whether a small disturbance of relative intensities at the two ears would upset the judgment of any observer listening in the open. Unknown to him, a piece of board could be introduced near his ear on the same side as the sound, by which it was supposed that the intensity on that side would be reduced, perhaps even below the intensity upon the farther side. It was found that no misjudgment of direction could thus be induced. Although not actually conclusive, this result is unfavourable to a theory that would make the lateral sensation dependent upon small differences of intensities. The repetition of the experiment by other observers, and in varied forms, seems to be desirable.

We come now to the third assumption (*c*). The theory under discussion assumes that two vibrations arriving at the inner ear from opposite directions co-operate if the phases are opposite, and tend to neutralise one another if the phases are the same. But this depends entirely upon the precise mode of action. The assumption is justified if the effect depends upon the velocity of vibration, as when aerial waves impinge from opposite directions upon a sensitive flame. But if the question were the effect upon a resonator, of the kind employed by Helmholtz, the direction of travel is indifferent, and co-operation in all cases requires that the phases be the same.* So far as I see, we have nothing to guide us in choosing between the two alternatives.

And there is a further question. If we are to suppose that the framework of the skull is thrown into vibration by sounds which enter at either ear, the matter is not exhausted when we have included one travel across the head. If a finite system, free from viscosity, is excited by an incident force following the harmonic law, the phases of the resulting vibration are the same (or the opposite) at all points of the system. On this basis the phase-difference β, on which everything depends, disappears. This conclusion may be evaded by appealing to viscous effects, but then it becomes a question whether the propagation across the head represented by g would suffice. It is difficult to form a confident opinion.

Further evidence, if it can be obtained, is very desirable. For the moment, the choice between the competing views is likely to depend upon preconceptions as to the manner in which the nerves act. And it should be remembered that the question is not whether phase-differences at the two outer ears are competent to arouse the lateral sensation, but only as to the manner in which this effect is produced.

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* 'Phil. Mag.,' vol. 7, p. 153, 1879; 'Theory of Sound,' 2nd edit., vol. 2, p. 403.